

result which we cannot get by continuous application of the ice bag.

Dr. W. F. B. Wakefield, San Francisco.—Recent experiments in Berkeley have shown that peristalsis can be stopped by the use of calcium chlorid. I think that is the basis of the Ochsner treatment.

Dr. E. Rixford, San Francisco.—Concerning my own paper, there is very little more to say except that inflammation of the appendices epiploicæ is far more common than ordinarily supposed. I have seen no reference to it in the literature. Obscure inflammatory trouble on the left side will be explained in this way. I would like to say a word in favor of the Ochsner treatment in cases of acute perforated appendices with rapid-spreading peritonitis. About Ochsner's statistics of only 4 per cent—Ochsner says that his mortality in acute perforated cases is only 4 per cent, but I find that he counts as a perforated appendix all sorts of cases that I have not considered in that class; that is, anything in which the material has gotten out of the appendix even with microscopical perforation. Also with Mayo. I think that if Ochsner would confine his statistics to those cases in which there is a progressive peritonitis that his mortality would be vastly higher than 4 per cent. Still I believe very strongly that where you have an advancing peritonitis, Ochsner's plan of rest is best. My own experience, which has been small, has been in favor of it. The difficulty is, in the individual case, to use that common sense which Dr. Barbat speaks of. The difficulty is whether the individual case is going on to rapid-spreading peritonitis or not. It depends upon many factors. One is the virulence of the germ. The pneumococcus is bad. A solution of the problem will depend upon the diagnosis and interpretation of the blood pressure, leukocytosis and distention and various other symptoms.

Dr. A. W. Morton, San Francisco.—With regard to the statistics of Dr. Deaver, he refers to all of his cases. If he figures up the acute cases you find he has 14 per cent of mortality. If you remember the discussion at Saratoga between Ochsner and Deaver you can readily see the animosity in that discussion. Deaver said, "Ochsner, in Chicago, is dealing with a different class of cases." In the same discussion at New Orleans, between Murphy and Ochsner, they had the same time. They are different as day and night. There are two forms of treatment. One of them invariably operates whenever he makes a diagnosis of appendicitis. He does not even use common sense. Common sense is what Ochsner's treatment is. He operates in all cases if there is doubt in the condition; if it has passed into that stage where there is general peritonitis, or just before you get to peritonitis, where you have an effusion; that is, a rupture of the appendix into the peritoneal cavity. If you keep down the peristalsis and keep the patient quiet it will always be walled off. That is the key of his treatment. As for gastric lavage, Dr. Ochsner does not claim to have originated that theory. He simply advocated it and brought it to the attention of the profession. There is one point in his treatment that you will find difficult; that is, feeding a patient. If you leave the patient at home without a trained nurse, some friend will give him beef tea or egg-nog, and break up the whole theory. You will not be able to carry out that treatment unless you remove the patient to a hospital or have a private nurse. As to the time for the operation, I like the theory of Mayo. He does not go as far as Ochsner does. Just as soon as the condition of the patient improves, or he believes that the abscess is walled off, he operates. It is an excellent thing to do at that time.

Dr. W. I. Terry, San Francisco.—Ochsner has certainly made his impression upon this country. Dr. Morton says one thing in his paper and another thing in his discussion. In the paper he says, give water

by mouth, and give enemas. In discussion he says give entire rest.

Dr. Van Zwahlenburg, Riverside.—With regard to what Dr. Barbat said about pus in the peritoneal cavity, it has been disproven time and time again. Many patients have recovered. The Ochsner treatment has been discussed pretty thoroughly. With regard to Dr. Lobingier: In the first place Dr. Lobingier differs very widely from myself; but I think he evades the question. To be sure, appendicitis is an inflammation, but it is not always the same inflammation. Not anywhere will you find such rapid changes of the same nature as in the appendix. The changes in the blood vessels are important. In twenty-four hours you can see many changes. There is a sudden change in the circulation that has been noted by all pathologists. This change is a mechanical one. This theory explains that change. The doctor says that I bring out a curiosity to prove my theory. If you will operate during the first two days you will find many cases distended like this one. Abby proves that many cases are found fully distended. In all cases where I operated early, where the blood had not disappeared, the plug was still present and the appendix distended. It is not a curiosity.

ON THE ACTION OF SALINE PURGATIVES.†

By JOHN BRUCE MACCALLUM, University of California, Berkeley.

IT HAS been commonly believed that saline purgatives act locally upon the intestines. According to Schmiedeberg, Cushny and others they have a purgative action because they prevent the absorption of fluid from the intestines. Loeb has stated that these purgatives are identical with the salts which produce contact irritability, muscular twitchings and hypersensitiveness of the nervous system; and he suggested that they increase peristalsis by increasing the irritability of the nerves and muscles of the intestines.

The following experiments show that the saline purgatives barium chlorid, sodium citrate, sulphate, tartrate, etc., act as purgatives not only when introduced into the intestines, but also when injected subcutaneously or intravenously. The intravenous injection of $\frac{1}{2}$ cc. M/8 sodium citrate in the rabbit causes marked peristalsis within one minute; $\frac{1}{2}$ to $\frac{1}{4}$ cc., M/8 BaCl₂, causes strong peristaltic movements.

It was found that in addition to the increased peristalsis caused by the subcutaneous or intravenous administration of the saline purgatives, there is to be noted also a marked increase in the secretion of fluid in the intestines. A considerable quantity of clear yellow fluid collects in the loops of intestines which before the injection were practically empty. Careful measurements of this secretion were made, and it was found that, in a loop tied off from the rest of the intestine and drained by a canula placed at one end of the loop, the quantity secreted in a unit of time was greatly increased after the administration of one of the saline purgatives.

It was found that both the increased peristalsis and increased secretory activity in the intestines could be brought about by the local application of solutions of the saline purgatives to the peritoneal surfaces of the intestine; 1 cc. M/320 BaCl₂ is sufficient when applied in this way, to start strong peristaltic movements. With sodium citrate, sulphate, etc., the concentration must be greater.

†Synopsis of remarks accompanying the demonstration of a number of experiments on the action of saline purgatives to the Alumni Association of the Medical Department, University of California. The results of these experiments have been published as follows: American Journal of Physiology, Vol. X, 1903, p. 101; Vol. X, 1904, p. 259. University of California Publications, Physiology, Vol. I, 1903, p. 5; Vol. I, 1904, p. 81; Vol. I, 1904, p. 115.

A further series of experiments showed that a loop of intestine entirely removed from the body and placed in a solution of one of the saline purgatives at body temperature, not only exhibits characteristic peristaltic movements, but also secretes fluid into the lumen. This shows that both peristaltic movements and secretion in the intestine may take place in a loop which is entirely separated from the central nervous system and is not supplied with blood.

It was further shown that the peristaltic movements as well as the increased secretory activity caused by the saline purgatives can be inhibited by the administration of calcium or magnesium chlorid. In connection with this it was shown that the increased secretion of urine caused by diuretics can be inhibited by calcium chlorid. The normal flow of urine can be diminished by the same means. Calcium chlorid is thus an antidiuretic.

It is therefore certain, as shown by these experiments, that the saline purgatives do not act locally in the intestine, but exert their influence after they have been absorbed into the blood, since they act more quickly and in smaller doses when introduced into the blood. For this same reason they do not act because they are secreted into the intestine. It is also shown that the main factor in the production of fluid or semi-fluid feces is not as Schmiedeberg claimed, the prevention of the absorption of fluid from the intestine, but on the contrary, it is the direct secretion of fluid into the intestine. The increased peristalsis carries the softer feces of the upper intestine into the rectum, while the increased secretory activity increases the amount of fluid contained by the intestine. The theory of Liebig that the purgative solutions act on account of their high osmotic pressure is entirely untenable in the light of these and many other experiments.

The increase of the muscular and glandular activities by the saline purgatives and their suppression by calcium chlorid is entirely analogous to the production and suppression of twitchings in voluntary muscles described by Loeb.

A number of possibilities suggest themselves with regard to the therapeutic use of these facts. It seems quite possible that the use of subcutaneous purgatives might be resorted to in some cases. Further, cases of persistent diarrhoea, especially those of nervous origin, might be greatly benefited by the administration of calcium chlorid. For the same reason, with rectal infusions of saline solution, which in some cases cannot be retained until they are absorbed, it might be of service to add to the infusion a small quantity of calcium chlorid. This would inhibit the peristaltic activity of the rectum caused by the NaCl solution which would therefore be retained. In cases of polyuria, especially those of nervous origin, calcium chlorid might be of distinct benefit. Attention must be called to the extremely poisonous nature of barium chlorid, and the production of muscular twitchings by sodium citrate when given subcutaneously. In the administration of calcium chlorid, care should be taken that the flow of urine is not suppressed.

EPINEPHRIN; THE ACTIVE PRINCIPLE OF THE SUPRARENAL GLAND; A SECOND WORD.

By PHILIP MILLS JONES, M. D., San Francisco.

I have received an abstract of a paper by H. Pauly, date of March 31st, 1904, with the request that the essential statements be given space. Pauly objects to the formula suggested by Abel ($C_{10}H_{13}NO_3 \cdot \frac{1}{2}H_2O$), and insists on the formula advocated by Takamine ($C_9H_{13}NO_3$). He says that "This formula (Abel's), together with the name 'Epinephrin Hydrate,' which designates the same, should be blotted out of the lit-

erature. The name 'Epinephrin,' however, should remain now as before for the basic substance obtained by treating adrenalin with concentrated sulphuric acid, or with dilute acids under pressure, and to this the formula $C_{10}H_{13}NO_3$ should be given." And again: "Thus there can be no doubt that not Abel and Crawford, but Takamine first obtained the active substance." This is the gist of Pauly's contention.

In the *Muenchener Medizinische Wochenschrift* for June 7th, 1904, there is a still later article on the same subject, by Emil Abderhalden and Peter Bergell. They find, as the result of recent investigations, that the probable formula is as given by Takamine ($C_9H_{13}NO_3$), but they disagree in almost every other particular from the conclusions of Pauly. In their opinion, neither Abel nor Takamine has succeeded in isolating the pure crystallin active principle, for solutions of the product of either threw down a sediment and did not keep. The preparation made by Abel they found to be slightly more impure than that made by Pauly, and all other preparations were still less pure, with the exception of that made by Dr. Heinrich Byk, of Berlin. Of this latter they speak in the very highest terms, and say that it is the only absolutely pure preparation of the crystallin body that they have observed. It throws down no precipitate and keeps in solution. Experimentally, they find a 1 to 1,200 or 1,300 solution of this preparation equal in strength to a 1 to 1,000 solution of the ordinary commercial brands, adrenalin, suprarenin, etc. This preparation of Byk's bears the name "Epiprenan," which is still another new one!

Now what is it all about? Why, about what we shall call the crystallin-active blood-pressure-raising-principle of the suprarenal gland. Obviously we cannot call it all that, and live. Nor can we call it adrenalin-adnephren-adrenamine-adrenol-adrin-taprenalin-hemisin-hemostatin-suprarenalin; life is too short. Abderhalden and Bergell use the name "Epinephrin," and do not demand that "it be blotted from the literature," as evidenced by the title of their paper, "Über das Epinephrin (Epiprenan)."

Epinephrin seems to be a pretty good name for this particular thing. Some years ago Abel discovered that a chemical acting like this did exist, though he had not isolated it, and he named it epinephrin. That sort of thing has happened many, many times, in chemical work, and is well known and accepted. Helium was known to exist as such, and was named, long before any one isolated it as an element; so with radium. And so, too, so far as the evidence goes, with epinephrin.

We are not squabbling over the exact chemical formula, nor over the fact as to whether or not any one of the commercial brands is absolutely pure; there are enough good chemists in the world now working on those points to settle the matter in the course of time. If very great commercial interests were not involved there would be no question about the name to be applied to the thing—epinephrin would be generally accepted and used, for it is as good as any other. But the trouble lies in the fact that there are numerous brands of the thing, each with a different and a controlled name, and each owner of a name and a brand wants his particular name used; this is what we object to. Some words (registered as trade-marks, and thus owned) have been so grafted into the language that it is hopeless to try to get them out. But that is no reason why the number should be increased, and so far as the editor of this one poor, little journal can help it, the number will *not* be increased. Epinephrin (or more accurately, epinephrin hydrate) is a good name; it is understood by all chemists to mean just exactly the thing indicated—the crystallin active principle, or blood-pressure-raising element of the suprarenal gland, and until the majority of chemists cease from using it in that sense, that is what it will be taken to mean.